the author still holds to the opinion that the formation is an Eocene one. A larger part of the work is occupied with descriptions of the Green River plants, chiefly from Florissant, an incredibly rich locality. This is prefaced by a lucid description of the beds, which exceed 300 feet in thickness, by Mr. S. Scudder. They are principally volcanic ash accumulated in one or more old lake-basins. These and most of the other fossiliferous rocks are situated towards the top of the Green River group, which is reckoned to be 2000 feet thick. The flora contains 228 species, of which 152 are from Florissant, and is referred by Lesquereux to the Oligocene. It was originally thought by him to be Miocene, but the detailed comparisons he has made between it and that described by Saporta from Aix, in Provence, prove that he is justified in putting its age further back. Indeed it bears a marvellous resemblance to that of Bournemouth, and had he been able to make comparisons he would perhaps have assigned it a still earlier date. It is a matter of the greatest interest to find in America a flora corresponding to those of Aix and Bournemouth, and not represented anywhere to the north. The last pages are occupied with descriptions of some new Miocene plants from various localities. The book is illustrated by fifty-nine coloured plates, and however we may differ as to the value of the determinations themselves, all will agree as to the great service rendered to science by the publication of such an important mass of data for future comparison.

J. STARKIE GARDNER

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Coal-Dust Question

ON several occasions during the last five years, Sir Frederick Abel has referred to the history of the coal-dust question, and to my connection therewith; and as his views on this subject do not altogether correspond with mine, I desire, with your permission, to state in this place how much, and in what particulars, I differ from him.

In his address to the Society of Arts, delivered on the 17th of November last, Sir Frederick says: "Several well-known French mining engineers published, many years after Faraday and Lyell wrote, observations and experimental results as new, which were simply confirmatory of those philosophers' original statements and conclusions, and to some extent this was also the case in still more recent publications in this country by Galloway and Freire-Marreco."

Faraday and Lyell's statements and conclusions were to the following effect:—

1. Fire-damp is not the only fuel in an explosion.
2. The coal-dust is swept up by the blast and is partially burnt.
3. (Speaking of Haswell Colliery explosion)[1] "There is every reason to believe that much coal-gas was made from this dust in the very air itself of the mine, *by the flame of the fire-damp*, which raised and swept it along, and much of the carbon of this dust remained unburnt only for want of air."

That is to say, the flame of the fire-damp is extended, and the effect of the explosion is aggravated by the presence of the coal-dust.

The Committee of the Coal Trade, who replied to Faraday and Lyell's report in a letter dated February 7, 1845, do not appear to have thought those authors' remarks about coal-dust of sufficient importance to be noticed at all. Nor do the authors themselves seem to have attached any particular importance to them save as a record of a curious physical and chemical fact; for after making them they immediately turned, like all their predecessors and most of their successors, to the contemplation of imaginary magazines of fire-damp as a means of accounting for the explosion. Moreover, both of them lived for many years afterwards, during which one great explosion occurred after another, and yet we do not find that either of them ever lifted so much as the tip of his little finger to point to coal-dust as the probable cause of the catastrophes.

In 1855, M. du Souich, Ingénieur des Mines, said,[1] "A sort of crust of light coke, which could be gathered from the timber at various points, could only have originated from the coal-dust swept up in the working places and carried to a distance by the extremely violent air-current caused by the explosion. This dust being itself partially inflamed *could continue the effects of the fire-damp by carrying them further*" (*peut continuer les effets du grisou en les portant plus loin*).

In 1861,[2] M. du Souich and M. Estaunié again insisted on the same thing in similar terms.

In 1867,[3] M. du Souich again developed the same opinions.

In 1864,[4] Verpilleux de Reydellet, A. Burat, Poumairac Baretta and other engineers emitted opinions similar to the foregoing.

In 1875,[5] M. Vital wrote, "Extremely fine coal-dust is a cause of danger in dry working places in which shot-firing is carried on; in well-ventilated workings it may of itself alone give rise to accidents; in fiery workings it increases the chances of an explosion, and when an accident does occur *it aggravates the consequences of the fire-damp flame*" (*coup de feu*).

In 1875,[6] MM. Desbief and Chansselle gave a short historical *résumé* similar to that of M. Haton, and quoted an opinion of M. Verpilleux which appears to resemble my own, but has never, so far as I am aware, been put prominently forward by the author nor supported by experimental or other proof, namely: "M. Verpilleux, who attaches great importance (*importance capitale*) to coal-dust, was one of the first to call attention to it; comparing a fire-damp explosion to the detonation of a gun, he went so far as to say that the dust represents the powder and the firedamp the priming."

In March, 1875, Sir Frederick, then Professor, Abel, addressing an audience at the Royal Institution on the subject of "Accidental Explosions," referred to explosions in mines, and mentioned the researches that had been made by Mr. R. H. Scott and myself up to that date. He also speaks of dust explosions in flour mills, &c.; but, as showing the small importance he attached to anything that had been previously said or done in regard to coal-dust, it is remarkable that he does not even refer to its existence.

I had been investigating the subject of great colliery explosions since the year 1870, but had been unable to discover any explanation of their occurrence wholly satisfactory to myself. At the commencement of my work I had read all, or nearly all, the English literature connected with it then extant, and amongst other things the report and article of Faraday and Lyell on the Haswell Colliery explosion, and the reply of the Committee of the Coal Trade;[7] but, so little impression did Faraday and Lyell's remarks about coal-dust make upon me at the time, that I afterwards forgot I had read them, and was only reminded of the fact by seeing it recorded in an old note-book of my own some time after the publication of my first paper. I retained the impression, however, for in the paper[8] referred to I wrote: "The accounts of colliery explosions published in this country hardly ever allude to the existence of coal-dust; and when they do so, in one or two cases [it should have been one case only] it is for the purpose of suggesting that the gases disengaged from it by the heat of the fire-damp flame would no doubt be ignited and tend to increase the force of the explosion."

It did not for a moment occur to me that this, which is Faraday and Lyell's view, could ever be accepted as an explanation of the phenomenon I was trying to elucidate.

---

[1] *Phil. Mag.*, 1845.

[1] "Rapport de M. Haton de la Goupillière : Des Moyens propres à prévenir les Explosions du Grisou," 1880.

[2] *Ibid.* [3] *Ibid.* [4] *Ibid*

[5] *Annales des Mines*, 1875.

[6] *Comptes rendus des Réunions mensuelles de Saint Etienne*, June, 1875.

[7] *Iron*, June 1, 1878. [8] *Proc. Roy. Soc.*, 1876.

© 1885 Nature Publishing Group

In the beginning of the year 1875 I removed to South Wales, and soon discovered that all the collieries in which great explosions had occurred were dry and dusty. My previous experience was almost wholly confined to damp mines, in all of which, with one exception (Nitshill), there was no coal-dust, and in none of which, with the same exception, a great explosion had ever occurred. The impression referred to above probably exercised a certain influence at that time in helping me to form the conception of what I believe to be the true explanation of great colliery explosions. In the words of my first paper[1] it is as follows: "If it could be shown that a mixture of air and coal-dust is inflammable at ordinary pressure and temperature, there would be no difficulty in accounting for the extent and violence of many explosions which have occurred in mines in which no large accumulations of fire-damp were known to exist; for it is only necessary to suppose that a violent gust of wind (originated, for example, by the explosion of a small accumulation of fire-damp) had swept through the adjoining galleries, raising a cloud of dust into the air, and then all the other phenomena would follow in regular order. The flame of the originally inflammable mixture would pass into the newly-formed one, expanding its volume; the disturbance would be propagated over an ever-widening area, until that area might possibly become co-extensive with the workings themselves; and *the consequences would be the same as if the whole space had been filled with an inflammable mixture before the disturbance began.*"

I demonstrated by an experiment first made on the 3rd of July, 1875, that air containing less fire-damp than can be detected by the ordinary means of testing the air in mines, is rendered inflammable at ordinary pressure and temperature when fine dry coal-dust is added to it. This was the first step towards proving the truth of my theory.

In the same paper I also stated, "It is always possible that if coal-dust could be made fine enough and were thoroughly mixed with dry air in the proportion of about 1 pound to 160 cubic feet of air, the mixture might be inflammable at ordinary temperature, or, *if not*, it might at least be so nearly inflammable that an explosion begun in it, in a confined space, might be propagated through it."

In my second (1879) and subsequent papers to the Royal Society, I stated the further opinion, first, as the result of more elaborate experiments, and secondly, as the result of a careful personal investigation and consideration of all the circumstances attending the occurrence of several great explosions, that, "A fire-damp explosion occurring in a dry coal-mine is liable to be indefinitely extended by the mixture of air and coal-dust produced by the disturbance which it initiates." This was the final step.

Of this, Sir Frederick says:[2] "Mr. Galloway was certainly the first to enunciate the conclusion that a small proportion of fire-damp is essential to impart to a mixture of air and coal-dust the power of propagating flame, though *he afterwards abandoned this conclusion in favour of the one some time previously accepted by Marreco*, to the effect that fire-damp is altogether unnecessary for the conveyance of flame with explosive effect by a mixture of dry coal-dust and air."

In January, 1878, I commenced a series of articles in *Iron* on coal-dust explosions, hoping thereby to arouse an interest in the subject, and, amongst other items of information, I gave translations of Vital's and Desbief and Chansselle's papers. Seven of these articles had already appeared when, in April, 1878, Prof. Marreco (to whom I had myself sent copies of them) and Mr. Morison, read their first paper before the Chesterfield Institute. They say, "Before the writers had the opportunity of learning what had already been accomplished abroad, their attention was directed to the subject by a paper read by Mr. W. Galloway before the Royal Society, in which he related his experiment in producing explosion in a mixture of fire-damp and air impregnated with dust. The opinion expressed in that paper, viz., that coal-dust was explosive only in an atmosphere containing a minute proportion of fire-damp, induced the writers to still further extend the scope of the experiments, and endeavour to discover whether coal-dust could by any means be exploded in an atmosphere totally free from fire-damp."

The arrangement to carry out the experiments here spoken of was made between Marreco and myself in the end of 1875 or beginning of 1876, and was referred to in my first paper (1876) thus: "It would be premature to draw any positive inference from my experiments with gunpowder shots, as they are by no means so satisfactory as I could wish, and I am glad to be able to state that Prof. Marreco, of Newcastle-on-Tyne, intends to investigate this question with more substantial apparatus."

Returning to Sir Frederick's remark, I would say that I abandoned no previous conclusion "in favour of one some time previously accepted by Marreco." I then held, and still hold, what is now a universally accepted opinion amongst mining men, that a mixture of air and fire-damp, which is not inflammable at ordinary pressure and temperature, may be rendered inflammable by the addition of dry coal-dust, and I was prepared, as soon as proof satisfactory to myself was forthcoming, to extend the area of my opinion, so as to include a mixture of fine dry coal-dust and air in the same category. This latter proof alone was necessary to the firm establishment of the theory enunciated at the commencement of my first paper, and for that reason Sir Frederick's statement appears to me to be wanting in logical sequence.

Neither the statement made by Marreco and Morison, that they intended to "endeavour to discover whether coal-dust could by any means be exploded in an atmosphere totally free from fire-damp," nor the results of the experiments published by them, appear to me to warrant Sir Frederick in taking up this position; for, concerning these very experiments of Prof. Marreco's he had himself remarked in the year 1880:[1] "It does *not appear that* in the numerous experiments made at Harton Colliery by the exposure of naked flames in currents of air laden with coal-dust, and by firing small cannon (representing blown-out shots) placed in various positions in such air-currents, *any indication has been obtained of a propagation of flame by the coal-dust.*" How then could Marreco have concluded that "fire-damp is altogether unnecessary for the conveyance of flame with explosive effect in a mixture of coal-dust and air"?

Sir Frederick continues to say, regarding myself: "Even his latest publication on the subject tends in the same direction, and emphasises his desire to 'claim that no earlier author had gone the length of crediting coal-dust with the *rôle* of principal agent, and relegating fire-damp to a secondary position.' The more recent results of other writers in this direction have, however, conclusively demonstrated that this is far too great a length to go, and that while on the one hand a very fine dry and highly inflammable coal-dust may, when raised and mixed with air by the force of a blown-out shot, become inflamed, and may then carry flame to considerable distances with a rapidity and violence of action similar to that of a fire-damp explosion, the extent to which, on the other hand, flame is propagated under corresponding conditions by most descriptions of coal-dust in the complete absence of fire-damp is very limited."

Sir Frederick and those whose opinions he here quotes appear to have omitted to take one element into consideration which has an important bearing upon the case. To this I may have occasion to refer more particularly at an early date.

W. GALLOWAY

Sunset-Glows

I WISH to call the attention of observers to a peculiar phenomenon which has been frequently noticed by me lately in connection with these sky-scenes. A very bright after-glow was visible here on October 27 last. I believe the date is correct, although I have unfortunately mislaid my day-book; the facts, however, I can vouch for otherwise, as they were detailed at the time. At 5 p.m. a heavy bank of cumulus was to be seen extending along the south-western sky-line, about 5° above and closely parallel to the horizon. High over this bluish bank of cloud rose the yellow haze of departing sunlight. This diffused after-glow was plainly intersected by numerous (I counted twenty-two) delicate streaks of nebulous stratus. These intersecting lines (they were scarcely "bands") were horizontal and parallel, piled up, as it were, above one another as high as 25° from the sea-line. The lowest hung apparently about half a degree above the gilded upper edge of the dark cumulus. On November 4, just before sunset, I exposed a gelatine plate, and succeeded in obtaining a photograph, in which seven of these narrow horizontal cloud-streaks can be faintly seen. On December 1 the following note was made:—4.20 p.m. Wind light,

---

[1] *Proc. Roy. Soc.*, 1876.

[2] *Journal Soc. Arts* No. 20, 1885, p. 23.

[1] "Report on the Results of Experiments made with Samples of Dust collected at Seaham Colliery, &c.," 1880.

© 1885 Nature Publishing Group